Greenwich, since the erection of the new transit instrument, and as computed by the solar tables of Delambre, which are used in the computation of the Nautical Almanac; with a view to the discovery of the errors in the elements of those tables. The number of observations from which this comparison was made is 1212, and they extend, with an interruption of only three months, from the end of July 1816 to the end of the year 1826. The result of the comparison at first indicated the necessity of a correction of the epochs of the sun's longitude, and of the longitude of the perigee, and perhaps also of the equation of the centre. But upon pursuing the examination through a series of years, it became manifest that some other source of irregularity existed, and that this could be no other than an erroneous estimate of the masses of some of the planets, especially of Venus and of Mars. A more critical examination showed, that there was also an error in the assigned mass of the moon.

The author proceeds to state the process by which he arrived at the determination of the amount of these several corrections. It was found necessary in these investigations to take into account an error which occurred in the tables with regard to the secular motion. It results from his researches, that the epochs for 1816 and those for 1821 to 25 ought to be increased respectively by $4''\cdot734$ and $5''\cdot061$; that of the perigee increased by $46''\cdot3$, and the greatest equation of the centre diminished by $0''\cdot84$. The mass of Venus should be reduced in the proportion nearly of 9 to 8, and that of Mars nearly in the proportion of 22 to 15. On a comparison of these results with those which have been derived from an examination of some of Dr. Maskelyne's observations, as given by Burkhardt in the *Connaissance des Tems* for 1816, they are found on the whole to agree in the most satisfactory manner. The principal discordance occurs in the correction of the place of the perigee; a discordance which the author thinks may arise either from want of correctness in the calculation of the term in the motion of the perigee, depending on the square of the time, or, what is more probable, from some undiscovered inequality in the formula, which is a function of the sun's mean longitude.

Experiments to determine the Difference in the Length of the Seconds Pendulum in London and in Paris. By Captain Edward Sabine, *of the Royal Artillery, Secretary of the Royal Society. Communicated by* Thomas Young, *M.D., Foreign Secretary to the Royal Society, and Secretary to the Board of Longitude.* Read November 15, 1827. [*Phil. Trans.* 1828, *p.* 35.]

The author commences this paper by a brief statement of the existing state of the determinations of standards of length in the two countries; and he observes an attempt made by M. Arago in 1817 and 1818, to bring into immediate comparison the standards of the two countries, proved inconclusive from the rates of the pendulums not having been obtained with sufficient exactness.

The author having obtained from His Grace the Master-general of the Ordnance a general leave of absence from his military duties so long as he could be usefully employed in scientific pursuits, conceived he could no way satisfy the condition better than in carrying into effect this purpose. Accordingly, being provided with two pendulums, one made for M. Schumacher, another the property of the Board of Longitude, he set out for Paris, whither the pendulums were forwarded to him.

The comparison was made in Paris at the Royal Observatory, in the Salle de la Méridienne, on the spot in which M. Biot's measurement had been made, and every proper facility and assistance afforded him. The coincidence-clock was compared every 12^h by M. Mathieu with the transit-clock of the Observatory. On the 27th of April, the weather having set in mild and steady, the experiments were begun. The results are stated in the form of appended tables, of which a detailed account is given. Table I. contains the daily rate of the clock used for the coincidences. Table II. contains the particulars of thirteen distinct determinations of the rate of the pendulum No. 8.; four by M. Mathieu; four by M. Nicollet; three by the author; one by Messrs. Nicollet and Savary conjointly; and one by M. Savary and the author. They are corrected as usual. Table III. contains the results of thirteen similar determinations of the rate of the pendulum No. 7, in which the author was also assisted by Messrs. Freycinet and Duperrey.

Each of the pendulums, when not used in observing coincidences, was employed in determining its rate by a journeyman-clock or counter,—a method used by Messrs. Freycinet and Duperrey, but which the author thinks inferior to that of coincidences, though capable of giving good results. The particulars of these are given in Tables IV. and V. From all these experiments in conjunction, it appears that the numbers of vibrations performed in a mean solar day at Paris (reduced as usual) by the two pendulums, were respectively 85922·06 and 85933·83.

The pendulums and apparatus were re-conveyed to London early in September by water, and the rates again determined at Mr. Browne's house in Portland Place, by means of that gentleman's excellent clocks, and transit observations made by Captain Sabine. The precautions used are fully detailed, and the observations, which are also appended in a tabular form in Tables III, VII, VIII, IX, X, XI, the author being assisted by M. Quetelet, of Brussels. They give as a final result 85933·29 and 85945·85 for the numbers of vibrations respectively, made by each in a mean solar day, similarly reduced for London.

As a final result of the whole operation, the author regards $12^s{\cdot}00$ as the acceleration of the seconds pendulum in passing from Paris to London. The same acceleration deduced from a comparison of M. Biot's and Captain Kater's direct measurements of the seconds pendulum, in Paris and in London, comes out 11·76, or, conversely, the length of the seconds pendulum observed by the former in London

transferred to Paris by an assumed retardation of 12^s, gives a length differing from M. Biot's by 0·00023. Borda's agrees within 0·00079 with M. Biot's, and Captain Kater's, so transferred, holds very nearly a mean between the two, but approaches rather nearer to Biot's than to Borda's.

On the Measurement of High Temperatures. By James Prinsep, *Esq. Assay Master of the Mint at Benares. Communicated by* Peter Mark Roget, *M.D. Secretary of the Royal Society.* Read December 13, 1827. [*Phil. Trans.* 1828, *p.* 79.]

The author, after adverting to the many abortive endeavours of former experimentalists to obtain instruments for the accurate admeasurement of high temperatures, and after suggesting doubts as to the confidence to which Wedgwood's pyrometer is entitled, describes several attempts of his own to effect this very desirable object. In the course of his inquiries, a remarkable fact presented itself to his notice in the change which occurred in an index constructed on the compensation principle, and formed by two slips of metal, the one of silver and the other of gold, originally quite pure, and united without any alloy. In the course of a few years, although it had never been subjected to a heat above that of melting lead, the whole surface of the gold became converted into an alloy of silver, the impregnation extending gradually to a considerable depth in the gold, and destroying the sensibility of the instrument to changes of temperature. After trying various plans, he gave the preference to one founded on the following principles: namely, that the fusing points of the pure metals are fixed and determined; that those of the three noble metals, namely, silver, gold, and platina, comprehend a very extensive range of temperature; and that between these three fixed points in the scale, as many intermediate ones as may be required may be obtained by alloying the three metals together in different proportions. When such a series of alloys has been once prepared, the heat of any furnace may be expressed by the alloy of least fusibility which it is capable of melting. The determinations afforded by a pyrometer of this kind will, independently of their precision, have the advantage of being identifiable at all times and in all countries: the smallness of the apparatus is an additional recommendation, nothing more being necessary than a little cupel, containing in separate cells the requisite number of pyrometric alloys, each of the size of a pin's head. The specimens melted in one experiment need only to be flattened under the hammer in order to be again ready for use. For the purpose of concisely registering the results, the author employs a simple decimal method of notation, which at once expresses the nature of the alloy, and its correspondence with the scale of temperature. Thus G .23 P would denote an alloy of gold with 23 per cent. of platina. As the distance between the points of fusion of silver and of gold is not considerable, the author divides this distance on the scale into ten degrees; obtaining measures of each by a suc-